The Effects of School Term Length on Education and Earnings: Evidence from a Regression Discontinuity Design

Rasyad Parinduri

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Rasyad A. Parinduri*

Abstract

This paper examines the effects of a longer school year in Indonesia on grade repetition, educational attainment, employability, and earnings. I exploit an arbitrary rule that assigned students to a longer school year in Indonesia in 1978-1979, which fits a fuzzy regression discontinuity design. I find the longer school year decreases the probability of grade repetition and increases educational attainment. It also increases the probability of working in formal sectors and wages later in life. Moreover, there is some evidence that some effects of the longer school year are larger for females and for individuals who grew up in rural areas.

Keywords: school term length, grade repetition, educational attainment, returns to schooling, regression discontinuity design, Asia, Indonesia
JEL classification: I21, J24, J31

* Nottingham University Business School, the University of Nottingham, Malaysia Campus, Jalan Broga, Semenyih, 43500, Selangor, Malaysia.
E-mail: rasyad.parinduri@nottingham.edu.my. Tel: +60-3-89248287. Fax: +60-3-89248019.

I. Introduction

Do children spend too little time in schools? What happens if governments extend term length: Does a longer school year facilitate learning and improve employability later in life?

Governments seem to have different answers to the question of the optimal term length: It varies across countries. Children in East Asian countries, for example, spend 208 days in schools on average in an academic year, much longer than children in the US do, 180 days (Lee and Barro, 2001). Indonesian children are in schools for 240 days, Korean 220 days, South African 195-200 days, British 190 days, Singaporean 187 days.[1]

This up-to-sixty-day difference begs the question of whether children in East Asia, or in developing countries in general, spend too much time in schools—a legitimate concern because the quality of schools in many developing countries is poor and these schools' educational inputs such as teachers and books are often inadequate. Therefore, spending too much time in these lousy schools is possibly just a waste of time. On the other hand, educational attainment in developing countries is low so that requiring the children to stay in schools longer may help them to learn more. The flip question is whether children in developed countries like the US spend too little time in schools, which may be one of the reasons why American school children do not perform as good as Asian

[1] The statistics of term length in Korea, UK, and South Africa are from INCA (2009); that of term length in Indonesia is from DPPJ (2012); that of term length in Singapore is from my own calculation based on MOE (2013).

and European children in science and math (IEA, 2007)—a concern that has been debated in the US in the past few decades (DE, 1983; DE, 1994).[2]

Early empirical works on term length do not find longer school years improve student performance. These papers exploit variations of term length across schools in the US (Grogger, 1996; Eide and Showalter, 1998); across US states (Card and Krueger, 1992; Rizzuto and Wachtel, 1980); and across countries (Lee and Barro, 2001; Wößmann, 2003). They use regression control strategy—ordinary least square or fixed-effect models. None of these papers uses exogenous variations to identify the effects of term length, however.[3]

Some recent studies such as Piscke (2007), Hansen (2008), and Fitzpatrick, Grissmer and Hastedt (2011) exploit natural experiments to identify the effects of term length. Piscke (2007), for example, uses variations in term length induced by the West Germany short school years in 1966-1967. Using fixed-effect models to estimate the counterfactuals, he finds the short school years increase grade repetition, but they do not affect earnings later in life. Fitzpatrick, Grissmer and Hastedt (2011) use variations in term

[2] President Obama himself has mulled over the idea of extending the school year in the US (Boston Globe, 2010). In recent years, a number of states in the US such as Arkansas, New Mexico, Iowa, and New Jersey have also tried to lengthen the school year to at least 190 days (New York Times, 2012; Record, 2012). According to the National Center on Time and Learning, as cited by New York Times (2012), about 170 schools in the US have extended their school year to 190 days or longer.

[3] See also Patall, Cooper and Allen (2010) for a survey of the literature on the length of school year in the field of educational psychology.

length induced by the timing of assessment dates, while Hansen (2008) exploits state-mandated changes in assessment dates in Minnesota and weather-related school day cancellations in Colorado and Maryland. The last two papers find longer school days improve student performance.[4]

In this paper, I exploit an arbitrary rule that assigned students to a longer school year in Indonesia in 1978-1979 to identify the effects of term length on educational and employment outcomes. Academic years in Indonesia used to start in January and to end in December the same year. In mid-1978, to synchronize academic years and government budget sessions, the then Indonesia's Minister of Education and Culture, Daoed Yusuf, decided to change the start of school year from the month of January to the month of July. To achieve this objective, he required schools in Indonesia to lengthen the 1978 academic year until June 1979. Children who were in schools in the 1978 academic year, therefore, did not complete their grades in December 1978, but rather they remained in the same grades for a six-month period until June 1979.[5]

Daoed Yusuf implemented this policy hastily. No curriculum changes were introduced; no major directives were issued. Teachers were not given new materials to be delivered in classrooms during the six-month extension; they were just asked to revise lessons covered in 1978. In fact, because of the haphazard implementation, parents associations and many education experts in Indonesia opposed this policy change.

[4] See also Marcotte (2007), Sims (2008), Marcotte and Hemelt (2008), and Llach, Adrogué, and Gigaglia (2009).

[5] See MPKRI (1978).

Nevertheless, Daoed Yusuf went ahead with this one-time term length extension so that, since 1979, academic years started in July and ended in June the following year.[6]

This longer school year fits a fuzzy regression discontinuity (RD) design: Most individuals who were born in 1972 or later did not experience the longer school year because they had not been in their schooling age when the government extended the length of the school year in 1978. Many individuals who were born in 1971 or earlier experienced the longer school year just because they were born one or a few years earlier and they were still in schools in 1978. This arbitrary assignment means that we can use the discontinuity in the probability of experiencing the longer school year between the 1971- and 1972 cohorts as an instrumental variable in a two-stage least square estimation of the effects of term length on educational and labour outcomes.

I find the longer school year decreases grade repetition and increases educational attainment: It increases educational attainment by 0.7 year—a large effect considering that the average educational attainment at the time is about nine years. It also increases the probability that an individual completed junior high and senior high schools by 15 and 21 percent, respectively.

The longer school year does not seem to increase employability, though there is some evidence that it increases the probability of working in formal sectors. The estimates of the latter are significant statistically if I define formality using information on the mode of payment or type of employers, but they are not if I use the information on whether jobs were under contracts or whether they were covered by pension plans

[6] See, for example, Tempo (1978).

The longer school year also increases earnings later in life. Using the basic specifications, I find the longer school year increases hourly wages by 13 percent on average—a large gain considering the increase in educational attainment is less than one year. Given that individuals who experienced the longer school year had also additional schooling of about six months, the 13 percent increase in wages translates into returns to education of about 8-15 percent.

I analyze the effects of the longer school year by gender because males and females possibly had different education and employment opportunities. I find the longer school year decreases females' probability of grade repetition by six percentage points or about forty percent, and the estimates are significant statistically; the effects on males, on the other hand, are small and insignificant statistically. Females also seem to gain more educational attainment than males do. There is also some evidence that the effects of the longer school year on females' earnings are higher than that on males' though the difference may not statistically differ from zero.

I also find different effects by the location where the individuals grew up. Individuals who grew up in rural areas benefit more from the longer school year in terms of lower probability of grade repetition and higher educational attainment. The effects on wages are higher in urban areas, though the difference may not differ statistically.

I do a number of robustness checks. I use additional control variables and alternative polynomial functions of the assignment variable. I also use alternative assignment variables and definitions of the longer school year. Overall, the results are robust. Some falsification tests also show no discontinuities in individual characteristics or

educational inputs in the early 1970s that may compromise the identification of the effects of the longer school year using the RD design.

This paper contributes to the literature in three ways. One, I provide the causal effects of the length of school year using an RD design, which is more transparent and more credible than difference-indifferences.[7] Two, I focus on a developing country where the effects of term length may differ from those in developed countries analyzed in the literature. Three, I examine the effects of the longer school year on both educational and labour outcomes, which complements the few papers in the literature that look at the effects of term length on labour outcomes later in life. This paper then provides some evidence on the employment effects of term length in the context of a developing country whose average educational attainment is low.[8]

The paper proceeds as follows. Section II describes the empirical strategy and the data. Section III discusses the estimates of the effects of the longer school year on educational outcomes and labour outcomes later in life. Section IV presents some robustness checks. Section V concludes.

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[7] See, for example, van der Klaaw (2008), Imbens and Lemieux (2008), and Lee and Lemieux (2010). To the best of my knowledge, no papers have used an RD design to identify the effects of term length on educational outcomes or labour outcomes later in life.

[8] Piscke (1997) and Llach, Adrogué, and Gigaglia (2009) are two of the few papers that examine the effects of term length on earnings later in life using exogenous variations of term length.

II. Empirical Strategy and Data

A. Empirical Strategy

I exploit an arbitrary rule that assigned students to a longer school year in Indonesia in 1978-1979, which fits a regression discontinuity (RD) design, to identify the effects of term length on educational and employment outcomes. Most Indonesians who were born in 1972 or later did not experience the longer school year because they had not entered primary schools when the government extended the length of the school year in 1978. Most individuals who were born in 1971 and some in the older cohorts experienced the longer school year just because they were born one or a few years earlier.

I use a fuzzy RD design because treatment to the longer school year is not deterministically assigned: Almost all individuals in the 1972 or younger cohorts did not experience the longer school year; not all individuals in the 1971 or older cohorts did because some of them dropped out of schools before 1978. There is, therefore, a discontinuity in the probability of experiencing the longer school year conditional on the year of birth, which I will use as an instrumental variable for the treatment status, the *longer school year*.

I describe the fuzzy RD design as follows.[9] Let D_i denote the treatment status, an indicator of whether an individual i experienced the longer school year. Then, we can write the probability of treatment for individual i as

[9] See, for example, Angrist and Pischke (2009) for a description of fuzzy RD designs.

(1) $$P(D_i = 1|yob) = \begin{cases} g_1(yob_i) & if \quad yob < 1972 \\ g_0(yob_i) & if \quad yob \geq 1972 \end{cases}$$

where $g(yob_i)$ is a function of yob_i, the *year of birth* of individual i, and $g_1(yob_i)$ and $g_0(yob_i)$ differ at the discontinuity in 1972. A dummy variable, *older cohorts* (T_i), which switches off at the discontinuity so that

(2) $$T_i = \begin{cases} 1 & if \quad yob < 1972 \\ 0 & if \quad yob \geq 1972, \end{cases}$$

can be used as an instrumental variable for D_i in a two-stage least square (2SLS) estimation.

The first stage of the 2SLS estimation is

(3) $$D_i = \alpha + \beta T_i + f(yob_i) + \varepsilon_{1i}$$

where $f(yob_i)$ is a polynomial function of yob_i. The fuzzy RD reduced form is

(4) $$y_i = \mu + \pi T_i + f(yob_i) + \varepsilon_{2i}$$

where y_i is a measure of educational or employment outcomes. We can then estimate the effects of the longer school year using the second stage of the 2SLS estimation

(5) $$y_i = \rho + \sigma \widehat{D}_i + f(yob_i) + \varepsilon_{3i}$$

where $\widehat{D}_i$ is the predicted values of treatment status from the first-stage regression, equation (3).

If the longer school year facilitates learning and improves employability, we expect the coefficient of T_i in equation (4) and that of $\widehat{D}_i$ in equation (5) to be positive for educational attainment, employability, and wages; they are negative for the probability of grade repetition.

B. Data

I use the Indonesia Family Life Survey (IFLS)—an on-going longitudinal household survey in Indonesia conducted by the RAND Corporation. The survey is a representative sample of about 83 percent of the Indonesian population and includes over thirty thousand individuals.[10] I use the latest wave of the survey, IFLS-4, which was done in 2007.[11]

I include individuals who were born in the 1960-1987 period to make sure that there were some likelihood that the older cohorts experienced the longer school year in

[10] The data is downloadable from http://www.rand.org/labor/FLS/IFLS.html.

[11] See Frankenberg and Karoly (1995), Frankenberg and Thomas (2000), and Strauss et al. (2009a) for descriptions of the IFLS. RAND collaborates with the Center for Population and Policy Studies of the University of Gadjah Mada and Survey METRE to do the IFLS-4.

1978-1979 (i.e., they were still in either primary, junior high, or senior high schools in 1978 if they did not drop out of schools earlier), and that the younger ones were old enough in 2007 so that their measures of educational and labour outcomes are reliable (i.e., the individuals who were born in 1987 would have completed senior high schools in 2007 when the survey was done). In most specifications, I have about 18,500 observations. Some specifications such as wage regressions have smaller number of observations, about 12,000, because some individuals were unemployed or out of the labour force in 2007. (See Table 1 for the summary statistics of key variables.)

I define *older cohorts* (T_i), the instrumental variable, equals one if individual *i* was born in 1971 or earlier, and zero otherwise. Most individuals who were born in 1972 or later entered primary schools in 1979; therefore, they did not experience the longer school year. Individuals who were born in 1971 or earlier experienced the longer school year if they were still in schools in the 1978-1979 academic year. In most specifications, I have about six thousand individuals whose T_i equals one and about twelve thousand whose T_i equals zero.

I construct the *longer school year* (D_i), the treatment variable, from information on the year of birth of individual *i*, her educational attainment, and the number of times she repeated grades. For the basic specifications, I define D_i equals one if individual *i* was in a primary, junior high, or senior high school in 1978 and zero otherwise. I assume all individuals in the 1972 cohorts entered primary schools in 1979 or later, which is a plausible assumption because most children in Indonesia enter primary schools in the year they turn seven years old. Therefore, for all individuals in the 1972 or younger cohorts, D_i

equals zero by definition; for individuals in the 1971 or older cohorts, D_i equals one if individual *i* was still in school in 1978 and zero otherwise. About two-third of individuals in the 1960-1971 cohorts experienced the longer school year; none of the individuals in the 1972-1987 cohorts did.

Because some children entered primary schools at the age of six or eight, in some specifications I define D_i using information on the year individual *i* entered a primary school. Therefore, D_i equals one if individual *i* entered a primary school in 1978 or earlier and remained in the school in 1978; it equals zero otherwise.

I prefer to use the year of birth to define the longer school year because the year of birth in the IFLS is more reliable than the year of entry to primary schools. In developing countries like Indonesia, some people do not know their birthdates, let alone the years they entered primary schools.[12] However, as part of robustness checks, I use the year of entry to primary schools to define the longer school year in some specifications.

I use eight educational outcomes: *repeating grades*, *repeating grades in primary school*, *educational attainment*, *completed primary school*, *completed junior high school*, *completed senior high school*, *able to read newspapers*, and *able to write letters*. All these variables are dummy variables except *educational attainment*.[13]

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[12] Even the year of birth of an individual in the IFLS may differ across books within a wave so that RAND has to make "best guesses" of the birthdates of some individuals using an algorithm to make them as consistent as possible (Strauss et al., 2009b).

[13] *Repeating grades* equals one if an individual repeated grades at least once. *Repeating grades in primary school* equals one if an individual repeated grades at least once while

I use eight employment outcomes later in life: *worked for pay*, *in the labour force*, *worked in formal sectors*, *worked under contracts*, *worked and covered by pensions plans*, *worked and had wages paid per week or month*, *the logarithm of hourly wage*, and *the logarithm of monthly wage*. All these are dummy variables except wages.[14]

<Insert Table 1 here>

The summary statistics in Table 1 shows individuals in the sample were 32 years old and had nine years of schooling on average. About one in five repeated grades at least once; about 83, 60, and 42 percent completed primary, junior high, and senior high schools, respectively; most were able to read newspapers and write letters. About two in three individuals were in the labour force and most worked in informal sectors. Their average hourly wages were Rp 3,500 in 2007 rupiah, which is about US$ 0.38.

Simple comparisons between the older (i.e., 1960-1971) and younger (i.e., 1972-1987) cohorts do not indicate the favourable effects of the longer school year. The older

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she was in a primary school. The definitions of the other measures of outcomes are clear from their names.

[14] *Worked for pay* equals one if an individual worked for pay during the previous week. She was *in the labour force* if she either worked for pay or looked for jobs. She was *in the formal sectors* if she was a self-employed with permanent workers, government worker, or private worker. The definitions of the other measures of outcomes are clear from their names.

cohorts had lower educational attainment compared to the younger cohorts did, though the former were less likely to repeat grades; a larger proportion of them were unable to read newspapers and write letters (Panel B). The older cohorts also had mixed employment outcomes compared to the younger cohorts: The older cohorts were more likely to work for pay, but they were less likely to work in formal sectors or under contracts; the earnings of both groups were quite similar, however (Panel C).

III. Results

A. The First-stage Regressions

Figures 1.A and 1.B illustrate the first-stage regression of the treatment status, the *longer school year*, on the assignment variable, the *year of birth*. It plots the proportion of individuals who experienced the longer school year in 1978-1979 by the year of birth. The vertical dash line indicates the year after which almost all individuals did not experience the longer school year: To the left of the vertical line, some individuals experienced the longer school year; to the right, almost nobody did. The figure also fits a cubic polynomial of year of birth that may jump between the 1971- and 1972 cohorts. In Figure 1.A I use the year of birth to define the longer school year; in Figure 1.B the year of entry to primary schools.[15]

[15] I get similar pictures if I fit a quadratic- or quartic polynomial of year of birth. I present the cubic polynomial fits because the quadratic- and quartic functions seem to underfit and overfit the data, respectively.

<Insert Figure 1 here>

Figure 1.A shows an increasingly larger proportion of the individuals experienced the longer school year: from about one in five individuals in the 1960 cohort to almost all in the 1971 cohort. None of the 1972 or younger cohorts experienced the longer school year by definition. Figure 1.B in which I use the year of entry to primary schools to define the longer school year shows a similar picture: The proportion jumps from about 0.7-0.8 for the 1971 cohort to about 0.2 for 1972 cohort. This jump in the probability of treatment between the 1971 and 1972 cohorts suggests that we can use this discontinuity as an instrumental variable for the treatment into the longer school year.

Table 2 presents the first-stage regressions—the estimates of the fall in the probability of treatment from regressions of the *longer school year* on *older cohorts* and a set of controls. In column (1), I include year-of-birth cubic polynomial as controls. In column (2) I add age cubic polynomial and in column (3) I add gender and ethnicity dummies further. In Panel A I use the year of birth to define the longer school year; in Panel B the year of entry to primary schools.

<Insert Table 2 here>

The estimates confirm the discontinuity we see in Figure 1: Regardless of whether I include age, gender, or ethnicity dummies as additional controls, the probability of

experiencing the longer school year declines by about 100 percent between the 1971 and 1972 cohorts if I use the year of birth to define the longer school year, which means that the RD reduced form estimates would be similar to the 2SLS estimates of the effects of the longer school year. If I use the year of entry to primary schools to define the longer school year, the probability declines by 88 percent.

B. The Effects on Educational Outcomes

Figure 2.A illustrates the reduced form estimate of the effects of the longer school year, which I define using year of birth, on grade repetition. It plots the proportion of individuals who repeated grades by the year of birth. The figure also fits a cubic polynomial of year of birth that may jump between the 1971 and 1972 cohorts.

<Insert Figure 2 here>

The polynomial fit suggests that the 1972 cohort had a higher proportion of individuals who repeated grades compared to the 1971 cohort. About 14 to 18 percent of individuals who were born in the 1960s repeated grades at least once; the proportion rose to about 20 percent for individuals born in 1972 or shortly after, which indicates that the longer school year decreases the likelihood of grade repetition by about two percentage points.

Figure 2.B presents an analogous graph for educational attainment. It shows that, even though the educational attainment had been increasing from about six years in 1960 to ten years in the late 1980s, the average of educational attainment falls between the 1971 and 1972 cohorts by about one year. This fall means the longer school year increases educational attainment by about one year, which equals the ratio between the decline in educational attainment in Figure 2.B at the discontinuity and the fall in the likelihood of treatment in Figure 1.A.

Figure 2.C shows a similar graph for the proportion of individuals who had twelve years of education or more (i.e., completed senior high schools). The discontinuity of the proportion between the 1971 and the 1972 cohorts are even more obvious than that in Figure 2.B. The fall suggests that the longer school year increases the likelihood an individual had at least twelve years of education by about ten percentage points.

Table 3 presents the estimates of the effects of the longer school year on grade repetition, educational attainment, and other educational outcomes. Panel A shows the effects of the longer school year on the probability of repeating grade; Panel B educational attainment and the probability completing primary, junior-high, or senior high school; Panel C language skills. Column (1) provides the reduced form RD estimates; column (2) the 2SLS estimates. In all specifications, I use year-of-birth cubic polynomial as controls.

<Insert Table 3 here>

Panel A shows that the longer school year decreases the probability of repeating grades. Regardless of whether I consider grade repetition in all types of schools or in primary schools only, I find similar estimates—three percentage points. Considering that eighteen percent of individuals repeated grades at least once, experiencing the longer school year decreases the probability of repeating grades by about seventeen percent.[16]

Panel B suggests that the longer school year increases educational attainment by 0.7 year (row (6)). I also find the longer school year increases the probability an individual completed primary, junior high, or senior high school. (See rows (3-5).) The 2SLS estimates in column (2) suggest that the longer school year increases the likelihood of completing primary, junior high, and senior high schools by about 2, 9, and 9 percentage points, respectively. These estimates are significant statistically except the estimate of the effects on primary school completion. Economically, these effects are also large, in particular for high schools, because only 60 and 42 percent of individuals in the sample graduated from junior-high and senior high schools (see Table 1), respectively, which means the longer school year increases completion rates by 15 and 21 percent for junior high and senior high school, respectively.

Panel C shows the longer school year also improves language skills. The longer school year increases the likelihood an individual is able to read newspapers or write letters by about 0.3-1.1 percentage point. Because most individuals in the sample are able to read

[16] The 2SLS estimates in column (2) are similar to the reduced-form estimates in column (1) because the fall in the estimate of *older cohorts* in the first-stage regression is close to one.

newspapers and write letters, the magnitude of the effects are, therefore, small, only about 0.3-1.2 percent increase. Moreover, they are not significant statistically at the conventional level of significance.

C. The Effects on Labour Outcomes Later in Life

Figures 3.A-3.C present analogous graphs for the RD reduced form estimates of the effects of the longer school year on employment outcome later in life: whether an individual worked for pay, whether she worked in formal sectors, and her logarithm of monthly wage.

<Insert Figure 3 here>

Figures 3.A and 3.B show that more of the older generations worked for pay, and more of the younger generations worked in formal sectors: The polynomial fit in Figure 3.A is decreasing while in Figure 3.B increasing. There seems to be no clear decline in Figure 3.A between the 1971 and 1972 cohorts, but there is a slight decline in Figure 3.B. The longer school year does not seem to increase the likelihood that an individual worked for pay, though she may be more likely to work in formal sectors.

Despite the large variations of the averages in Figure 3.C, there seems to be a fall in the average monthly wages between the 1971- and 1972 cohorts. The logarithm of monthly wages was about 13.4 for the 1971 cohort and about 13.2-13.3 for the 1972 cohort, which suggests that the longer school year increases monthly wages by about 10-20 percent.

Wages are increasing to the left of the vertical dash line, and also increasing initially to the right of the vertical line, because older individuals tend to have higher wages. Wages are then declining because individuals who were born in the 1980s had just entered the job markets and, hence, had limited working experiences.

Table 4 presents the estimates of the effects of the longer school year on labour outcomes, which is analogous to Table 3. Panel A shows the effects of the longer school year on employment status; Panel B job characteristics; Panel C wages.

<Insert Table 4 here>

The longer school year seems to affect wages and some measures of formality, but there is no evidence that it improves employability. Though the estimates of employability are positive, they are small and insignificant statistically (rows (1-2)). The effects of the longer school year on formality are mixed: It seems to improve the probability an individual worked in formal sectors or paid per week or month, but it does not affect whether jobs are under contracts or covered by pension plans (rows (3-6)). Even though the effects on formality are mixed, the longer school year increases earnings later in life (rows (7-8)): It increases hourly and monthly wages by 13 and 17 percent, respectively.

IV. Robustness Checks

I do a number of robustness checks. One, I use additional control variables and alternative polynomial functions of the assignment variable. Two, I use alternative assignment variables and definitions of the longer school year. Three, I analyze the effects by sub-sample. Four, I do some falsification tests.

Table 5 shows the estimates of the effects of the longer school year on key outcome measures using additional controls and alternative polynomial functions of the assignment variable. In column (1) I add age cubic polynomial; in column (2) age cubic polynomial, gender, and ethnicity dummies; in columns (3-4) year-of-birth quadratic and year-of-birth quartic polynomials, respectively. The results are robust; the magnitude of the effects is also stable across the different controls.

<Insert Table 5 here>

Then, I check whether the results are robust to the use of quarter of birth instead of year of birth as the assignment variable. Figure 4 shows that, despite the noisier proportions and averages by quarter of birth, the trends of the proportion of individuals who repeated grades, that of the proportion who completed twelve years of education, and that of the averages of wages are similar to those in Figures 2-3. Using the quarter of birth as the assignment variable, I still find the longer school year affects educational and employment outcomes.

<Insert Figure 4 here>

Panel A of Table 6 presents the results. The table also presents two other specifications: using the year of birth as the assignment variable and the year of entry to primary schools to define the longer school year (Panel B), and using the year of entry both as the assignment variable and to define the longer school year (Panel C). Overall, the results are robust. There are differences only when I use the year of entry to primary schools as the assignment variable and I define the longer school year using the year of entry (row (3)): The effects on educational attainment, the probability of completing high schools, and wages are smaller. As I discuss in the Empirical Strategy and Data section, these smaller estimates are probably caused by measurement errors in the year of entry to primary schools, which pull down the estimates towards zero. In any case, the three estimates remain large economically and significant statistically.

<Insert Table 6 here>

Table 7 shows the effects by sub-sample to allow the longer school year affects different groups differently. Panel A presents the estimates by location of the individuals when they were twelve years old; Panel B by gender; Panel C by type of schools when they experienced the longer school year. Overall the estimates are robust, though there are some differences. One, the effects of the longer school year on grade repetition are larger for individuals who lived in rural areas and for females. Two, its effects on females'

educational attainment and probability of completing high school are larger. Three, there is also some evidence that the longer school year has larger effects on wages of females, individuals who lived in urban areas, and those who experienced the longer school year when they were in junior high schools, though the differences may not always statistically differ from zero.

<Insert Table 7 here>

In Table 8 I present some falsification tests to see whether individual characteristics and educational inputs decline between the 1971 and 1972 cohorts. I consider the effects of the longer school year on age of the individuals, the probability of whether an individual lived in rural areas when she was twelve years old, the number of hours in schools per day, the time it took for a one-way trip to schools, and the number of students in classrooms. In column (1) I use the year of birth to define the longer school year; in column (2) the year of entry to primary schools.

All estimates are insignificant statistically, which suggests that the use of the RD design to identify the effects of the longer school year is valid. There were no discontinuities in the age of the individual, flows of migration, educational inputs, and transportation costs during the early 1970s that may compromise the identification of the effects of the longer school year using the RD design.

<Insert Table 8 here>

V. Conclusions

The longer school year increases educational attainment and improves employment outcomes later in life. The RD design, exploiting an arbitrary rule that assigned students to the longer school year in Indonesia in 1978-1979, shows that the longer school year increases educational attainment by 0.7 year on average and the probability of completing junior high and senior high schools by 15 and 21 percent, respectively. The longer school year also increases hourly earnings later in life by 13 percent.

These results are different from the findings of Piscke (2007) who concludes that the short school years in Germany do not affect total education and earnings later in life. The results differ perhaps because educational attainment in Germany was higher than that in Indonesia. Moreover, the returns to schooling in Germany are zero (Piscke and von Wachter, 2005); the returns to schooling in Indonesia are high, about 6.8 to 10.6 percent (Duflo, 2001). In effect, if we include the six-month extension of the academic year in 1979, the longer school year in Indonesia in 1978-1979 increases the time spent in schools by 1-1.5 years, which means the effects of the longer school year on earnings corresponds to returns to schooling of about 8-15 percent, a figure that is higher than, but comparable to, Duflo's (2001) estimate of the returns to education in Indonesia.

The longer school year also decreases grade repetition, which is in line with findings in the literature. The magnitude of the effects is economically large: Experiencing the longer school year decreases the probability of grade repetition by three percentage points, which equals seventeen percent decrease. This effect is in particular larger for

females, six percentage points.[17] Females in the 1970s in Indonesia had poorer access to educational resources and had less favorable education opportunities than males did. When the females experienced the longer school year, they capitalized the schooling opportunity and many of them progressed to higher grades.

The different effects of the longer school year by gender also appear in educational attainment and employment outcomes later in life. There is some evidence that females gained higher educational attainment (0.96 year for females versus 0.54 for males) and earnings (26 percent versus 14 percent), though the differences may not always statistically differ from zero.

Even though the longer school year does not seem to affect employability, it improves the probability of working in formal sectors. The estimates of the effects on formality are positive and large in all specifications, which indicate that the longer school year matters. The statistical significance of the estimates varies from one definition of formality to another, however. The estimates are large and significant statistically if I define formality using mode of wage payment, but they are insignificant if I use contracts.

The novel findings that the longer school year increases educational attainment, the probability of working in formal sectors, and earnings later in life imply school children in developing countries where educational attainment is low like in Indonesia in the 1970s benefit from having longer school years. The results also suggest that the very long term

[17] This result is interesting because females during the period of analysis were ten percentage points less likely to repeat grades than males: Fourteen percent females repeated grade; 24 percent males did.

length in some developing countries, more than 200 days, is probably not too long. However, we cannot say much about whether term length in developed countries like Germany or the US is too short because educational attainment and school quality in these countries are much higher than those in Indonesia in the late 1970s.

It is important to note that these results are the effects of a longer school year that was planned hastily and done haphazardly. A poorly implemented longer school year in developing countries whose educational attainment is low can improve education and employability; a more carefully done term-length extension possibly delivers larger improvements in educational outcomes as well as labour outcomes later in life.

It would be good to know the mechanisms through which the longer school year increases educational attainment and earnings later in life. One possible mechanism is the longer school year allowed school children to revise lessons during the six-month extension, which increased the stock of human capital, however small it was. Another mechanism is teachers, because they had more time to teach, were able to spend more time to help weak students. Yet, another possible mechanism is parents valued the education of their children higher because their children did better in schools, in particular those who lived in rural areas. Perhaps, the most interesting mechanism is the effects on females: The longer school year provides more resources and education opportunities for females, which lowers grade repetition among females and translates later in life into higher educational attainment and earnings. The question on which of these mechanisms is the most important could perhaps be explored in future research.

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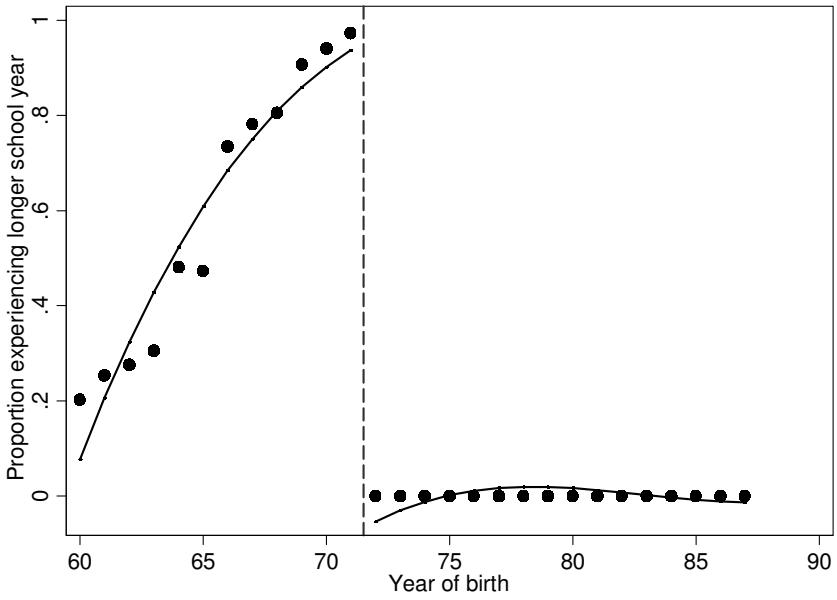
Figure 1: Proportion of Individuals who Experienced the Longer School Year

A. Using the year of birth to define the longer school year



B. Using the year of entry to primary schools to define the longer school year

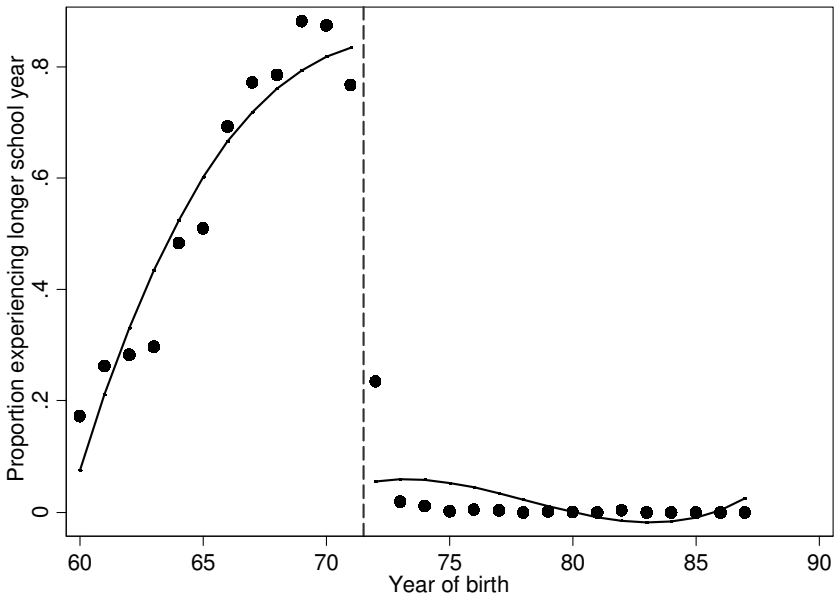
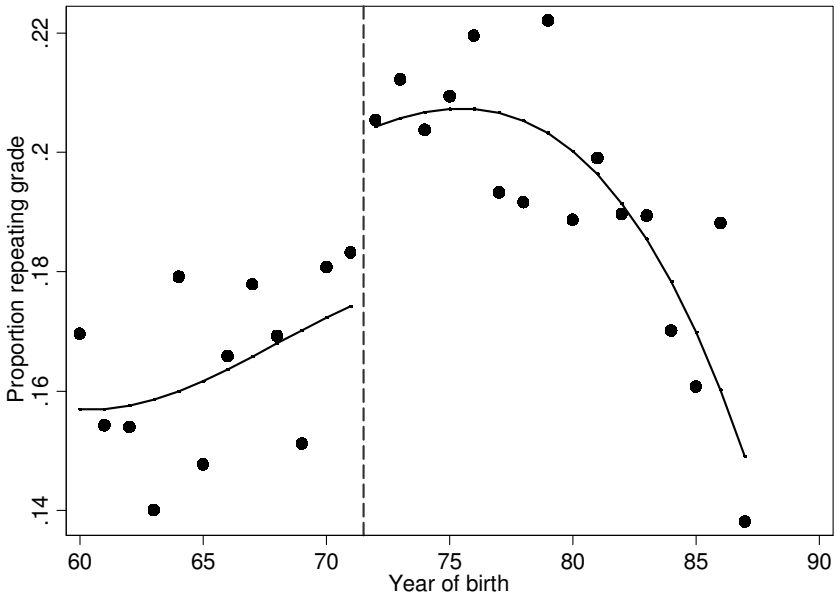
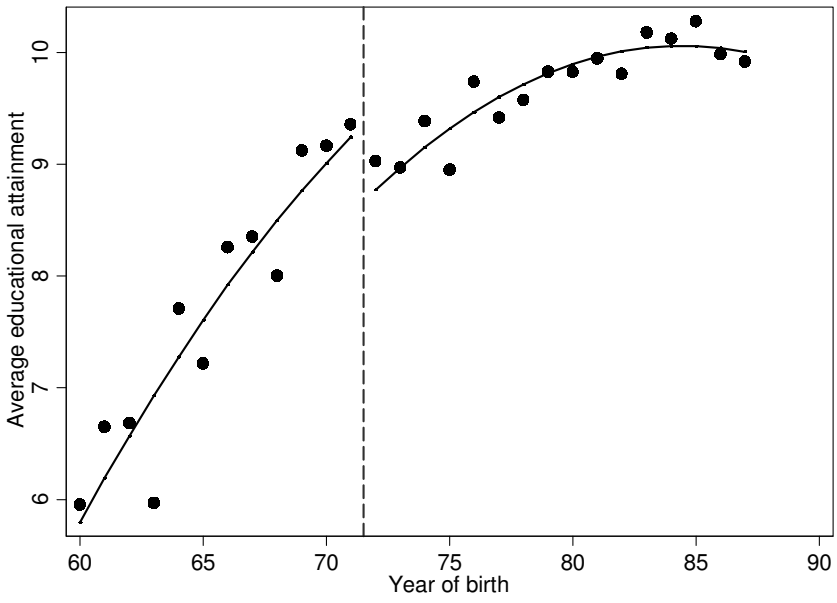


Figure 2: The Effects on Educational Outcomes

A. Proportion of individuals who repeated grades



B. Educational attainment



C. Proportion of individuals who completed twelve years of education

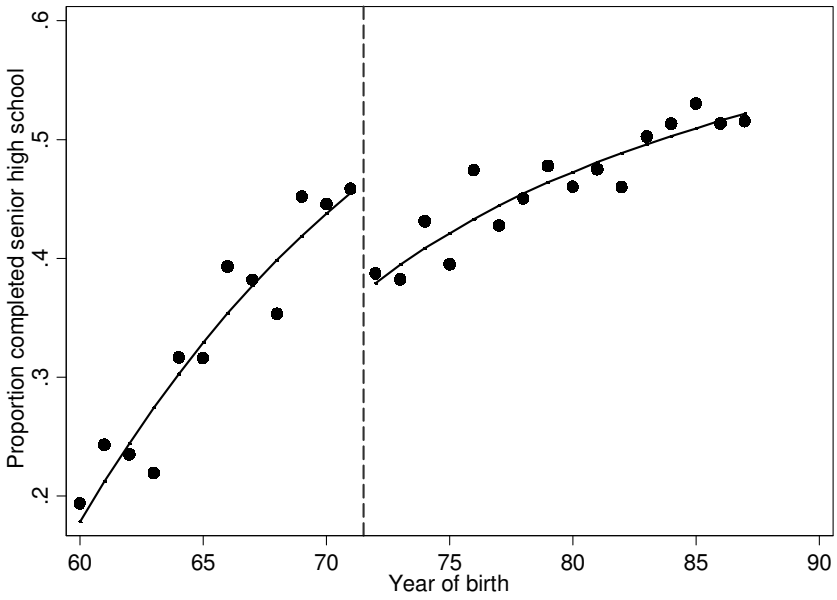
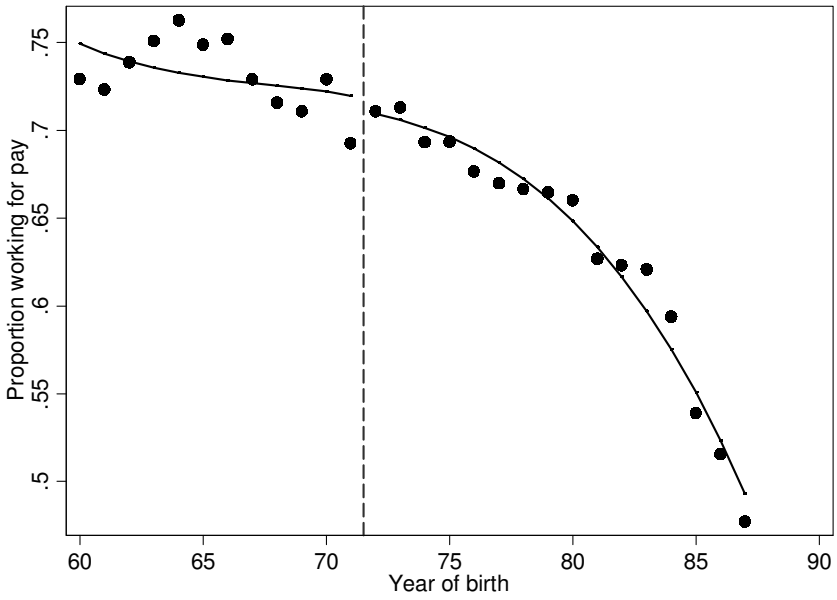
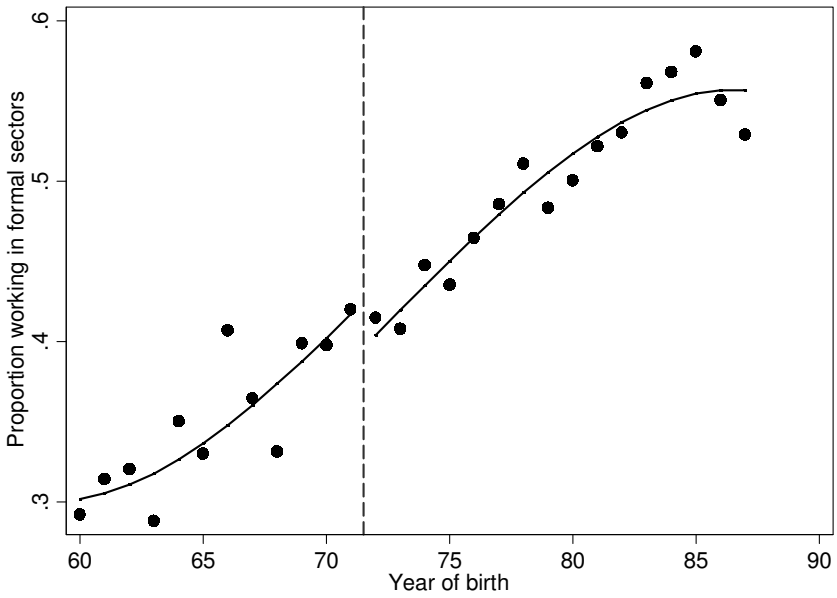


Figure 3: The Effects on Employment Outcomes

A. Proportion of individuals who worked for pay



B. Proportion of individuals who worked in formal sectors



C. Average Logarithm of Monthly Wage

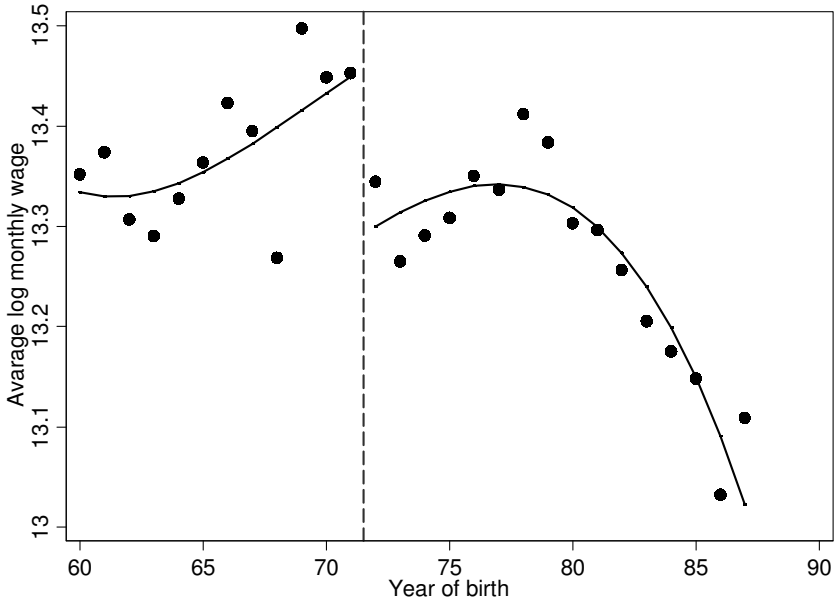
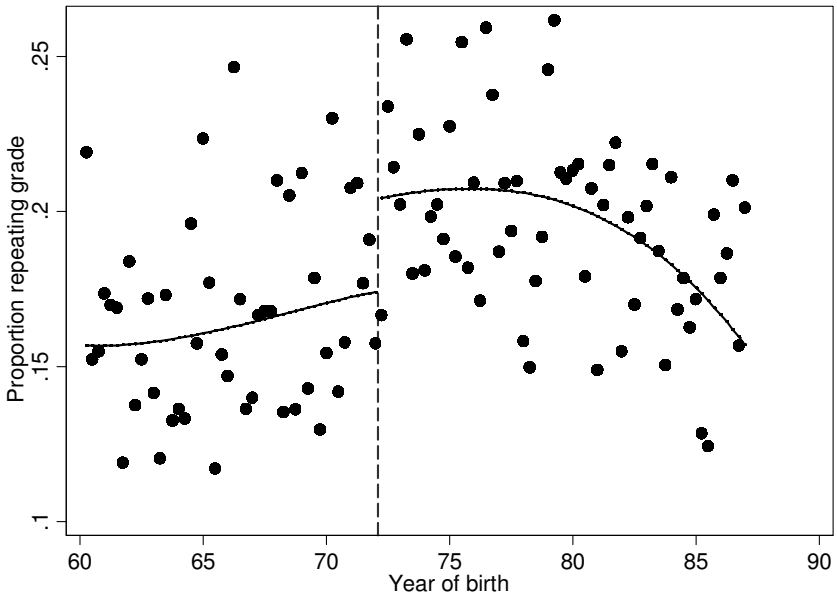
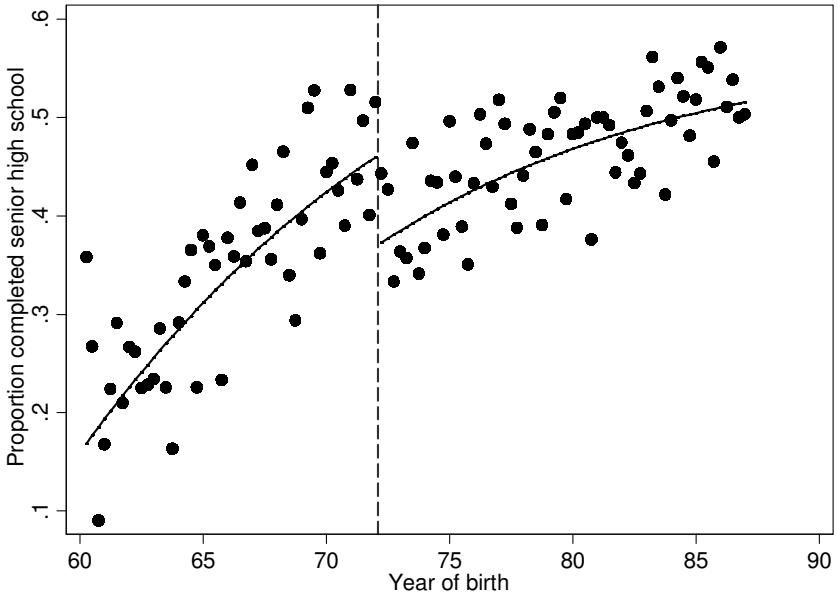


Figure 4: Using Quarter of Birth as the Assignment Variable

A. Proportion of individuals who repeated grades



B. Proportion of individuals who completed twelve years of education



C. Average Logarithm of Monthly Wage

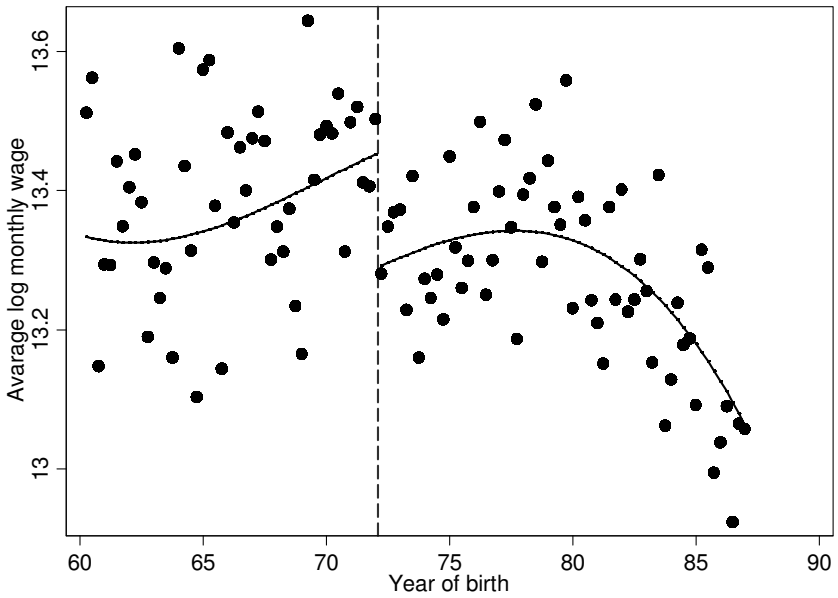


Table 1: Summary Statistics

Variable	1960-1971 cohorts	1972-1987 cohorts	1960-1987 cohorts
A. Control variables			
Male	0.48	0.47	0.48
	(0.50)	(0.50)	(0.50)
Age	41.28	27.51	32.14
	(3.47)	(4.42)	(7.70)
B. Educational outcomes			
Repeating grades	0.16	0.19	0.18
	(0.37)	(0.39)	(0.39)
Repeating grades in primary school	0.15	0.18	0.17
	(0.36)	(0.39)	(0.38)
Educational attainment	7.77	9.71	9.05
	(4.84)	(3.84)	(4.30)
Completed primary school	0.70	0.90	0.83
	(0.46)	(0.29)	(0.37)
Completed junior high school	0.46	0.67	0.60
	(0.50)	(0.47)	(0.49)
Completed senior high school	0.34	0.46	0.42
	(0.47)	(0.50)	(0.49)
Able to read newspapers	0.87	0.97	0.94
	(0.33)	(0.16)	(0.24)
Able to write letters	0.86	0.96	0.93
	(0.35)	(0.19)	(0.26)
C. Employment outcomes			
Worked for pay	0.73	0.64	0.67
	(0.44)	(0.48)	(0.47)
In the labour force	0.74	0.66	0.69
	(0.44)	(0.47)	(0.46)
Worked in formal sectors	0.35	0.50	0.44
	(0.48)	(0.50)	(0.50)
Worked under contracts	0.13	0.18	0.17
	(0.34)	(0.39)	(0.37)
Log of hourly wages	8.22	8.12	8.16
	(1.21)	(1.04)	(1.11)
Log of monthly wages	13.38	13.28	13.31
	(1.15)	(1.02)	(1.07)

Notes: The number in each cell is the mean; the figures in parentheses are standard deviations.

Table 2: First-stage Regressions

Dependent variable: Longer school year		(1)	(2)	(3)
A. Using year of birth to define longer school year				
Older cohorts	(1)	1.02	1.02	1.02
		(0.04)	(0.04)	(0.04)
Adjusted R^2		0.67	0.67	0.67
Number of observations		18,584	18,584	18,584
B. Using year of entry to define longer school year				
Older cohorts	(2)	0.88	0.88	0.88
		(0.11)	(0.11)	(0.11)
Adjusted R^2		0.65	0.65	0.65
Number of observations		16,735	16,735	16,735
Controls				
Year-of-birth cubic polynomial		✓	✓	✓
Age cubic polynomial			✓	✓
Gender and ethnicity dummies				✓

Notes: The number in each cell is the estimate of *older cohorts* from a separate regression of *longer school year* on *older cohorts* and a set of control variables. In Panel A, *longer school year* equals one if an individual was born in 1971 or earlier and was still in schooling in 1978; it equals zero otherwise. In Panel B, *longer school year* equals one if an individual entered a primary school in 1978 or earlier and was still in schooling in 1978. *Older cohorts* equals one if an individual was born in 1971 or earlier. The figures in parentheses are robust standard errors clustered by year of birth.

Table 3: The Effects on Educational Outcomes

Dependent variable		Reduced form	2SLS
		(1)	(2)
A. Grade repetition			
Repeated grades	(1)	-0.03	-0.03
		(0.01)	(0.01)
Repeated grades in primary school	(2)	-0.03	-0.03
		(0.01)	(0.01)
B. Schooling			
Completed primary school	(3)	0.02	0.02
		(0.02)	(0.02)
Completed junior high school	(4)	0.09	0.09
		(0.02)	(0.02)
Completed senior high school	(5)	0.09	0.09
		(0.02)	(0.02)
Educational attainment	(6)	0.69	0.67
		(0.23)	(0.21)
C. Language skills			
Able to read newspapers	(7)	0.003	0.003
		(0.02)	(0.02)
Able to write letters	(8)	0.01	0.01
		(0.02)	(0.02)

Notes: The number in each cell in column (1) is the estimate of *older cohorts* in a regression of an educational outcome on *older cohorts* and the year-of-birth cubic polynomial. Each cell in column (2) is the corresponding two-stage least square estimate. The dependent variables are listed on the left column. *Older cohorts* equals one if an individual was born in 1971 or earlier. The figures in parentheses are robust standard errors clustered by year of birth.

Table 4: The Effects on Employment Outcomes

Dependent variable		Reduced form	2SLS
		(1)	(2)
A. Employment			
Worked for pay	(1)	0.01	0.01
		(0.01)	(0.01)
In the labour force	(2)	0.003	0.002
		(0.01)	(0.01)
B. Type of jobs			
Worked in formal sectors	(3)	0.03	0.03
		(0.01)	(0.01)
Worked under contracts	(4)	0.02	0.02
		(0.02)	(0.02)
Worked and covered by pension plans	(5)	0.04	0.04
		(0.03)	(0.03)
Worked and wages paid per week or month	(6)	0.06	0.06
		(0.02)	(0.02)
C. Wages			
Log of hourly wages	(7)	0.13	0.13
		(0.04)	(0.04)
Log of monthly wages	(8)	0.17	0.17
		(0.05)	(0.04)

Notes: The number in each cell in column (1) is the estimate of *older cohorts* in a regression of a labour outcome on *older cohorts* and the year-of-birth cubic polynomial. Each cell in column (2) is the corresponding two-stage least square estimate. The dependent variables are listed on the left column. *Older cohorts* equals one if an individual was born in 1971 or earlier. The figures in parentheses are robust standard errors clustered by year of birth.

Table 5: Using Additional Control Variables and Alternative Polynomial Functions of the Assignment Variable

Dependent variable		(1)	(2)	(3)	(4)
Probability of repeating grades	(1)	-0.03	-0.02	-0.04	-0.02
		(0.01)	(0.01)	(0.01)	(0.01)
Educational attainment	(2)	0.68	0.65	0.62	0.76
		(0.21)	(0.18)	(0.17)	(0.22)
Completed senior high school	(3)	0.09	0.09	0.09	0.10
		(0.02)	(0.01)	(0.01)	(0.01)
Worked in formal sectors	(4)	0.03	0.02	0.003	0.02
		(0.01)	(0.01)	(0.01)	(0.01)
Log of monthly wages	(5)	0.17	0.17	0.07	0.18
		(0.04)	(0.03)	(0.04)	(0.04)
Controls					
Year-of-birth quadratic polynomial				✓	
Year-of-birth cubic polynomial		✓	✓		
Year-of-birth quartic polynomial					✓
Age cubic polynomial		✓	✓		
Gender and ethnicity dummies			✓		

Notes: The number in each cell is the two-stage least square estimate of *longer school year*. The dependent variables are listed on the left column. The figures in parentheses are robust standard errors clustered by year of birth.

Table 6: Using Alternative Assignment Variables and Definitions of Longer School Year

Dependent variable		Repeating grade	Educational attainment	Completed high school	Formal sectors	Log of monthly wage
		(1)	(2)	(3)	(4)	(5)
A. Assignment variable: quarter of birth						
Longer school year (using year of birth)	(1)	-0.03	0.65	0.09	0.02	0.17
		(0.01)	(0.30)	(0.03)	(0.02)	(0.04)
B. Assignment variable: year of birth						
Longer school year (using year of entry)	(2)	-0.03	0.76	0.11	0.02	0.18
		(0.01)	(0.16)	(0.02)	(0.01)	(0.04)
C. Assignment variable: year of entry						
Longer school year (using year of entry)	(3)	-0.02	0.31	0.05	0.02	0.10
		(0.01)	(0.21)	(0.02)	(0.02)	(0.05)

Notes: The number in each cell is the two-stage least square estimate of *longer school year*, which is defined using year of birth or year of entry to primary schools. Each regression includes quarter-of-birth-, year-of-birth-, or year-of-entry cubic polynomial. The dependent variables are listed on the top row. The figures in parentheses are robust standard errors clustered by year of birth or quarter of birth.

Table 7: The Effects by Sub-sample

Dependent variable		Repeating grades	Educational attainment	Completed high school	Formal sectors	Log of monthly wage
		(1)	(2)	(3)	(4)	(5)
A. By location at twelve years old						
Rural	(1)	-0.04	0.74	0.11	0.02	0.16
		(0.02)	(0.20)	(0.02)	(0.02)	(0.08)
Urban	(2)	0.003	0.67	0.10	0.02	0.22
		(0.02)	(0.14)	(0.02)	(0.04)	(0.05)
B. By gender						
Males	(3)	0.003	0.54	0.06	0.02	0.14
		(0.02)	(0.23)	(0.03)	(0.02)	(0.08)
Females	(4)	-0.06	0.96	0.15	0.02	0.26
		(0.02)	(0.17)	(0.02)	(0.04)	(0.10)
C. By type of schools						
Primary schools	(5)	-0.02	0.69	0.16	0.01	0.18
		(0.02)	(0.24)	(0.02)	(0.03)	(0.08)
Primary- or junior high schools	(6)	-0.03	0.65	0.13	0.02	0.24
		(0.01)	(0.11)	(0.02)	(0.02)	(0.05)

Notes: The number in each cell is the two-stage least square estimate of *longer school year*. Each regression includes the year-of-birth cubic polynomial. The dependent variables are listed on the top row. The samples are described on the left column. The figures in parentheses are robust standard errors clustered by year of birth.

Table 8: Falsification Tests

Dependent variable		(1)	(2)
Age	(1)	-0.01	-0.01
		(0.01)	(0.01)
Lived in rural areas when twelve years old	(2)	-0.02	-0.02
		(0.02)	(0.02)
Number of hours in schools per day	(3)	0.01	0.03
		(0.06)	(0.08)
Time it took for a one-way trip to schools	(4)	0.63	0.71
		(0.51)	(0.61)
Number of students in classrooms	(5)	-1.35	-1.14
		(1.05)	(1.54)
Variable used to define longer school year			
Year of birth		✓	
Year of entry			✓

Notes: The number in each cell is the two-stage least square estimate of *longer school year*, which is defined using year of birth or year of entry. Each regression includes the year-of-birth cubic polynomial. The dependent variables are listed on the left column. The figures in parantheses are robust standard errors clustered by year of birth.